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ADVISORY MEDICAL BOARDS FOR INSANE ASYLUMS.

[Communicated for the Boston Medical and Surgical Journal.]

IN the number of this JOURNAL for October 13th appeared a paper read before the Suffolk District Medical Society, and one of a series, concerning "The Medical Management of Insane Women," in which it is attempted to show the necessity and also the great utility of a Medical Board of Advisors to our Insane Asylums, for the benefit of the patients, the hospitals, the superintendents, the profession and the community.

The writer of that paper says, "his remarks are based upon extended observation, patient reflection," &c. It is quite possible if his observation could be a little further "*extended*" he would not only discover the uselessness of such a board, but the positive disadvantages of adding any more official boards to our already liberal allowance of disinterested guardians of the public charitable institutions for the insane. He is not aware, it seems, of the privilege granted superintendents by the trustees, and expressly and wisely directed sometimes by the by-laws of our hospitals, to call in consultation at any time such physician or surgeon as they may think proper for special cases. It would seem to be much more in accordance with an intelligent understanding of the whole subject, to allow the superintendent, who may be supposed to know what he needs, to choose his advisor from the whole range of his professional brethren, than to have him confined to a board appointed, as it might be, for personal or political reasons. In the latter case he is *compelled to take* such advice as the appointed board may be *able and willing* to give. If, as the writer allows, the advisory board visits patients only at the request of the superintendents (and no other course would be tolerated by any competent superintendent), it is not difficult to see where the real advantage would be secured.

The writer of this article is familiar with the duties required of

physicians in insane asylums, having acted both as assistant and superintendent in one of the most important of our New England hospitals; and while so employed he had occasion to call consultations occasionally, and he can cheerfully testify of the aid and comfort so received. But in no instance did he feel the need of any *permanent* board of advisors, *such as is recommended.*

It is said above that the advisory board might be appointed for political reasons. It might not. But by what influence was the late board of "Commissioners in Insanity" selected? For what reason were all the well-known "experts," able men and true, omitted from that Board? Without any wish to intimate that said Board was deficient, either in talent, experience or accomplishment for the task required, it is sufficient to say that of the three constituting that Board, two only were physicians, that neither of these was ever heard of as an "expert" in insanity, or as being inside the walls of an insane asylum in any other capacity than as an accidental or curious visitor, before his appointment to examine and report upon the difficult and complicated subject of insanity.

But let us examine some of the points taken by the author of the paper referred to. 1st. The advantage of an advisory board to the patients.

He has not made it very clear how the *mere appointment* of an advisory physician presents any advantage over the existing arrangement, when he admits that said advisor shall be called upon only at the discretion of the superintendent. The difference would consist merely in the person thought fit by the Governor, or whoever had the power of appointment, to give advice, and the person thought by the superintendent, a medical man and capable of judging of all the circumstances as they may arise, most suitable to aid him in any particular case.

If patients are not treated "*secundum peritissimam artem*" in any hospital at the present day, that hospital is under the care of an incompetent medical superintendent, and the fault is in the man, and not in the want of a board of advisors who, if appointed, could not act unless *called upon.* And the testimony of the quotation from the letter of one superintendent, saying "he could not make such an examination as was necessary for a full understanding of the case," &c., is "conclusive" only of the lack of a *disposition* on his part to *make use* of all the means of treatment considered proper to aid in restoring his patient.

In regard to the risks to which superintendents are liable from the delusions of insane patients, an advisory board would not relieve them except to give a wider field for the speculator. The danger is just as great every day, and when nothing out of the usual order of practice is said or done, if *delusions* are sufficient to annoy or victimize the managers of insane asylums.

Of the advantages of an advisory board to the profession.

The establishment of advisory boards *would*, undoubtedly, "stimulate physicians to more frequent study of mental disease," and it would afford a larger number from which to select gentlemen for superintendents and trustees. So far there would be an apparent advantage. But if the holding of such an office should lead them to suppose themselves qualified thereby to take the charge of one of our hospitals containing three or four hundred unsound minds, the *disadvantage*, after a very short time, would be *very* apparent.

As an example of the feeling that might be engendered by holding such an office, let us quote a few lines from the paper *under treatment*:—

"As a single instance of what would naturally and generally be the result, it may be mentioned that shortly after the report of the late Massachusetts Commission in Insanity had been rendered, it was urged upon two of the three gentlemen constituting that Board, that they should allow their own names to be used as eligible to the vacant superintendency of the Northampton Asylum. Upon the third of their number similar arguments would have been used had he been a physician."

If either of the gentlemen *so urged* supposed for a moment that by yielding to the advice of his partial but imprudent friends he would have secured the countenance and support of a single member of the intelligent Board who had the power of appointment, he is certainly under a great delusion.

This opinion is based upon *extended observation*, and given for the benefit of any who may hereafter entertain similar sentiments, holding office bringing them slightly in relation to the subject of insanity. *

LABOR, WITH ARM PRESENTATION, FOLLOWED BY DEATH.

[Communicated for the Boston Medical and Surgical Journal.]

MESSRS. EDITORS,—The case of "Difficult Labor" reported in the last number of your JOURNAL, by Dr. S. Sargent, of Lawrence, is very similar in many of its features and its result to one which occurred in my own practice several years ago, and which I write out wholly from remembrance for your use. Its corresponding features will give it interest, though it may not aid much in elucidating Dr. Sargent's case.

Dec. 6th, 1860.—Mrs. W. was seized early in the day with labor pains. Not much progress was apparent till evening, when an arm protruded. After some ineffectual efforts to return the arm and deliver by turning, and anticipating trouble, owing to the excessive rigidity, and also influenced by some former painful experience, I had the late Dr. B. Cutter, of Woburn, sent for without delay to my aid. On his arrival, I administered ether to insensibility, and requested

him to effect the delivery. The rigidity was still extreme and the expulsive efforts severe, and he found much difficulty in effecting the turning and in bringing down the legs. He at length succeeded in delivering a stillborn child. The flooding meanwhile was large. The placenta came readily away, but she did not rally. Measures were used without success to stimulate and to promote respiration. She became considerably conscious, spoke feebly a few times, and died, but a short time after delivery and while we were both present.

Fearing internal difficulty as the fatal cause--rupture of the uterus, or hæmorrhage, or both—I the next day made an autopsy, and found the uterus entire and well contracted and no hæmorrhage whatever. My own conviction was that she had died from exhaustion caused by inordinate flooding and the severe manipulation to which she had been subjected. My friend Dr. Cutter was inclined to the opinion that her death had been caused by the *ether*. I could not concur, as the symptoms preceding did not seem to justify the belief. While she roused to consciousness, she continued to sink physically.

Many years ago, before the discovery of the anæsthetic qualities of ether, I witnessed the death of a woman in the hands of another physician, from ruptured uterus, caused by turning. In the absence of a *post-mortem* examination, we are left more in the dark as to the cause of death in Dr. Sargent's patient, but must presume, I think, it was internal hæmorrhage, and, probably, ruptured uterus.

Winchester, Oct. 29th, 1864. A. C.

HISTORIES OF TWO SURGICAL SPECIMENS IN THE ARMY MEDICAL MUSEUM, WASHINGTON, D. C.

[Communicated, by permission of the Curator, Surgeon J. H. Brinton, U.S.A., for the Boston Medical and Surgical Journal, by BURT G. WILDER, Asst. Surg. 55th Mass. Vols.]

CASE I.—Gun-shot wound of heart. Death. Specimen No. 2,639, Army Medical Museum. (Ventricular portion of heart, and conical pistol-ball.)

1st Lieut. Dennis H. Jones, 55th Mass. Vols., was wounded at Yellow Bluff, Fla., March 23d, 1864, at 5, P.M., by the accidental discharge of a Smith and Wesson revolver. The ball was conical, of the calibre of thirty-two one-hundredths of an inch. It entered the left breast between the 4th and 5th ribs. There was no wound of exit. Lieut. J. instantly fell backward, only exclaiming "Save me, save me" (from a supposed enemy, I thought). His face expressed mental anxiety rather than bodily pain.

His hands convulsively grasped a rope, but only for a moment, and, with one gurgling respiration, both heart and lungs appeared to cease to act within half a minute. A sort of groan, when the body was raised a few minutes afterwards, may be attributed rather to

the sudden displacement of the large quantity of fluid in the cavities than to any conscious voluntary action.

Sectio Cadaveris, at 8, P.M. *Rigor mortis* slight. Surface of the body very pale and quite cool. The single external wound was very small, and was situated two inches below and a little inward from the left nipple. There had been no external hæmorrhage whatever, but on opening the thorax more than three quarts of blood were found in its cavity and that of the pericardium, the lung being compressed against the anterior parietes.

From its entrance, the ball had passed almost directly backward, inclining towards the middle line, perforating the heart near its apex so as to leave two openings in both the pericardium and left ventricle, and had buried itself, base foremost, in the body of the 10th dorsal vertebra, but without entering the spinal canal.

Of the two wounds in the walls of the left ventricle, that of entrance is one inch above the apex of the organ, and close to the inter-ventricular septum; that of exit is one and three quarters inches above the apex and three eighths of an inch from the septum. The former is small, and one fourth of an inch in diameter throughout; the latter is one fourth of an inch in diameter at its inner extremity, but greatly increases in size from within outward, so as to be of a conical shape, with the outer edges somewhat torn. The *columnæ carneæ* on the septum are just grazed in one spot, whence it may be inferred that the ventricle was dilated when struck by the ball. The contrast between the two wounds suggests the idea that the inversion of the ball may have been caused by the sudden contraction of the walls of the ventricle at the instant of perforation. On the base of the ball are two slightly converging grooves, apparently made by contact with the bone.

Case II.—Simple fracture of cervical vertebræ. Compression of spinal cord. Death. Specimen No. 3,159, Army Medical Museum. (Cervical portion of spinal cord.)

John A. Tice, private, Co. I, 1st New York Engineers, aged 18 years. The patient was very robust and strong. While bathing at Folly Island, South Carolina, July 27th, 1864, he ran and dove from a beach into two feet of water, striking the hard sandy bottom with the top of his head. There was immediate loss of sensation and of motion below the shoulders, and in the upper extremities, except over and in the flexor muscles of the forearm (*biceps* and *brachialis anticus*), and, apparently, in the *deltoid*, though the extension of the arm was feeble and undeterminate. Consciousness was unimpaired. Respiration diaphragmatic. Circulation but little accelerated. Some, though not excessive, pain in the lower cervical region.

During the night he was restless and wakeful, and pulv. Doveri, gr. v., was given him.

18th hour.—Fever and some delirium. Magnesiæ sulphatis, ʒ i., a cathartic enema, and wet cups along the spine, No. xii., were pre-

scribed. The symptoms abated after a copious alvine discharge. There was some flatus, with incontinence of fæces, but not of urine; the catheter was used three times a day. Applications of ice were made to the head, and of mustard to the feet and thighs.

54th to 60th hour.--High fever and more delirium. Treatment continued.

He now became more quiet and easy, but asphyxia commenced and steadily increased until death, at the sixty-eighth hour. During the last eight hours he took cough mixture and spir. frumenti, aa ʒ i., *pro re natâ*. To the last moment there was inability to cough or forcibly expectorate. Power of speech failed about six hours before death, but he could understand when spoken to for four hours longer.

Sectio cadaveris, July 30th, 3, P.M., two hours after death. Body of 6th cervical vertebra found vertically fractured, the posterior edges of the partially separated halves pressing upon the anterior surface of the cord, opposite the origin of the 7th and 8th cervical nerves. The arch of the 6th vertebra was broken down, and there was dislocation of the spinous processes of the 4th, 5th and 6th, with sufficient fracture of their arches to injure the cord, including most of the roots of the 6th cervical nerve, but not those of the 5th nerve.

At the point of direct injury the cord was softened, inflamed and disorganized, so as to escape as a red pulp from an incision in the pia mater. The disorganization had also extended upward along the grey commissure, and the anterior horns of grey matter above the roots of the 4th cervical, but not apparently along the posterior horns.

The roots of the 5th cervical nerve, the upper one of the brachial plexus, being originally unhurt, motion and sensation remained to those parts supplied by the musculo-cutaneous nerve, the fibres of which appear to constitute the upper one of the outer cord of the plexus, so that the flexors of the forearm, which are supplied by this nerve, could act, and the long head of the biceps could also feebly extend the arm at the shoulder till ten or twelve hours before death. The origin of the phrenic nerve (from the 3d and 4th cervical) being untouched by the original injury, the diaphragm could continue its function of abdominal respiration till the inflammation invaded its roots, after which asphyxia became prominent and caused death in six hours.

DR. GAILLARD'S PRIZE ESSAY ON OZONE; ITS RELATIONS TO HEALTH AND DISEASE.

[Concluded from page 276.]

We have thus concluded the hygienic and therapeutic relations of ozone. We have shown that it is a deodorizer, a disinfectant; a diuretic, diaphoretic, stimulant, antispasmodic, astringent and hæmo-

static—a combination certainly rare and valuable, belonging to no other article that we can use, and demonstrating that any agent, thus presented to us, is worthy of the closest study and most general investigation. We will briefly consider the relations of ozone to physiology, before bringing our essay to a conclusion.

We find that an aqueous solution of ozone has the most striking effect upon organic substances. Dr. Richardson states, "that blood freed from fibrin absorbs oxygen from the peroxide, and if it is venous blood it becomes arterial, with a rise in temperature. Washed fibrin and cellular tissue, in the fresh state, evolve the oxygen; albumen, urea, gelatin, fibrous membrane and skin produce no change. Grape sugar, and indeed all the sugars, brought into contact with it, become decomposed and evolve carbonic acid. Starch undergoes the same modification. These observations refer to animal substances recently used, but when putrefaction has commenced, then the oxygen seems to act on all alike and to produce rapid disintegration. An interesting fact, relating to the peroxide, is that its oxidizing power is easily prevented by the presence of certain bodies, having a wide extension of names but analogous characters. Ammonia, or vapor in solution, tobacco, hydrocyanic acid, solution of aconite, and, in short, all narcotics that are miscible with water, possess this neutralizing property; the permanency of the result being decided by the character of the agent employed. This establishes the great law, advanced by Snow, *that narcotism is suspended oxidation*, and that every substance, which on being introduced into animals produces narcotization, has the property of preventing the union of oxygen with other substances with which it is in contact and for which it has an affinity. A weak solution of ozone oxidizes the blood, but this action can be stopped by the action of alkaloids and narcotics. Injected into the left side of the heart of an animal, it restores the irritability, but appears to have an opposite effect on the right side. Injected into the arteries, immediately after death, it seems to restore to the muscles the power of contraction, on the application of an irritant. It suspends to a considerable extent cadaveric rigidity, and it reduces the spasmodic action excited by such bodies as ammonia and hydrocyanic acid."

Dr. Richardson deserves the thanks of the profession for his interesting and valuable experiments in regard to the varied properties belonging to the aqueous solution of ozone. With a supply of pure peroxide of barium, any physician can manufacture his aqueous solution of ozone, and administer it, with as little difficulty as attends the solution of quinine, morphine, or any similar articles of the Dispensatory. Messrs. Bullock & Reynols, Pharmaceutists, of London, are now preparing the peroxide of barium for general use, and with this we can make every hygienic or therapeutic application of ozone that may be desired. After for many years measuring the amount of atmospheric ozone with the ozonoscope, examining its effects and

testing its properties, *we are at last enabled to offer an artificial preparation of this agent, for general use, to the profession.* We have a perfect ozonoscope; we can certainly now prepare perfect and uniform ozonometers; *and we have finally an efficient and always accessible preparation of the agent itself.* The field is all ready; the instruments all prepared; we ask, now, but that willing laborers shall come and contribute to the harvest. With a prepared and fruitful field, with perfect instruments for its tillage, competent workmen have only to unite to secure a harvest that shall be an ample reward for past and future labors.

Ozone has received the most enthusiastic attention from the savans of Europe; the list of observers is a long one, and exhibits some of the most familiar and distinguished names known to science. We find, in this catalogue, the names of Williamson, Marignac, De La Rive, Faraday; Schönbein, at Bâle; Wolf, at Berne; Simonin Sr. and Simonin Jr., at Nancy; Boeckel Sr. and Boeckel Jr., at Strasburg; Billiard, at Carligny; Robert, at Neudorff; Berigny and Silbermann, at Paris; Seitz, Slechner, Schnepf, Abt, Eschach and Knolz, at Vienna; Pourian, at Saulsaic; Decharmes, at Amiens; Grellois, at Constantinople; and very many others. The subject is studied at Athens and at Algiers; throughout France, England, Germany, Italy, Austria and the larger countries of the Eastern Continent. Results and tables are published at Paris, Munich, Strasburg, Berlin, Vienna, and wherever science is admired and appreciated in the olden country.

But what shall we say of America? What can we say? Near Montreal, Canada East, Smallwood quietly but industriously pursues his labors. Capt. Pope, U. S. Army, has made many and most valuable observations. With two, or perhaps three other names, the American list is ended. The Smithsonian Institute is, we believe, about to generally investigate the subject, but has so far almost entirely neglected it. Extensive in its ramifications, meritorious and efficient in all that it undertakes, it is to be hoped that this Institution will soon enter the field. "The wise men of the East" have done their duty; those of the West are lamentably recreant. In America, this subject is almost ignored, if not unnoticed; it forms the exception to the interest, study and investigation usually here bestowed upon subjects of scientific and practical import. Usually every new light which appears in the horizon of any science, if not increased here, is at least faithfully reflected. Why is it that this subject, so interesting, so important, so generally investigated by all who worship in the temple of science and stand about its portals, is here so generally neglected? Why is it that the American profession, characterized by its enthusiasm in the study and elucidation of scientific subjects, almost so universally ignores this agent; an agent so conspicuously active and efficient in the fields of pathology, therapeutics and hygiene? Why refuse to be co-laborers with those

distinguished philosophers of the olden world, who leave acts as epitaphs and works for monuments?

Where the nature and properties of an agent are to be farther elucidated, by a long-continued and diversified series of atmospheric tests, it is at least reasonable to expect that the American profession could render the most valuable aid. Scattered, as are its representatives, over the greater part of an entire continent, they can always contribute much that is interesting, and beyond doubt much that is important and original. The subject is no longer a chemical one in its characteristics, relations and attributes, but one of the most simple and practical character. To a nation whose distinguishing peculiarity is a study and culture of the practical, this subject, at last, offers every such attraction. Its habitat is the atmosphere, and who should more appropriately seek and examine it than those whose country represents the type of almost every zone, the illustration of each particular climate.

We have completed our subject.

We present these pages (if deemed worthy of presentation) to the profession, with the desire and hope that its representatives will see the importance and value of the interesting agent which forms their subject; that, by their united action, it may derive its chief elucidation as happily from the Western, as it did its birth from the Eastern Continent. Let it be everywhere represented in the armory of the profession; a profession which, Samaritan-like, is ever administering to the afflicted wayfarer, whenever found upon the roadside of Life.

AUTHORITIES UPON THE SUBJECT OF OZONE.

Memorada of Society of Natural History, at Bàle. Journal of Practical Chemistry. Annals of Poggendorff. Archives of Electricity. Annals of Liebig. Silliman's Journal. Braithwaite's Retrospect. Ranking's Abstract. Scientific Annuals. London Lancet Files. L'Oxygéne Electrisé, par H. Scoutetten. European and American Medical and Scientific Journals. Writings of Graham. Monographs, Researches, Letters and Tables of Schönbein, Boeckel Sr., Boeckel Jr., Williamson, Faraday, Richardson, Marignac, De la Rive, Frémy, Becquerel, Simonin Sr., Simonin Jr., Osann, Millon, Reiset, Baumertz, Marchant, Berzelius, Langlois, Seitz, Abt, Decharmes, Pourian, Grellois, Griffin, Boussingault, Bérigny, Wolf, Braconnot, Pelouze, Van Marum, Eschach, Knolz, Billiard, Slechner, Robert, Silbermann, Schnepf, Dutrochet, Shübler, Liebig, Fraas, Bergmann, Heller, Banal, Smallwood, Pope, Gregory, and many others. The names given represent those who have prominently identified themselves with this subject.

April 15th, 1861.

PROFESSOR HELMHOLTZ ON THE NORMAL MOTIONS OF THE HUMAN EYE.

(Concluded from page 285.)

THIS explanation relates only to Monocular vision; we have to inquire also how it influences Binocular vision.

Each eye has its field of vision, on which the visible objects ap-

pear distributed like the objects of a picture, and the two fields with their images seem to be superimposed. Those points of both fields of view which appear to be superimposed are called *corresponding* (or *identical*) points. If we look at real objects, the accurate perception of the superimposition of two different optical images is hindered by the perception of the stereoscopic form and depth; and we unite indeed, as Mr. Wheatstone has shown, two retinal images completely into the perception of one single body, without being able to perceive the duplicity of the images, even if there are very sensible differences of their form and dimensions. To avoid this, and to find those points of both fields of view which correspond with each other, it is necessary to use figures which cannot easily be united into one stereoscopic projection.

Two figures may be constructed, the right of which is drawn with white lines on black ground, the left with black lines on a white ground. The horizontal lines of both figures are parts of the same straight lines; the vertical lines are not perfectly vertical. The upper end of those of the right figure is inclined to the right, that of the left figure to the left, by about $1\frac{1}{4}$ degree.

Now I beg you to look alternately with the right and with the left eye at these figures. You will find that the angles of the right figure appear to the right eye equal to right angles, and those of the left figure so appear to the left eye; but the angles of the left figure appear to the right eye to deviate much from a right angle, as also do those of the right figure to the left eye.

When you draw on paper a horizontal line, and another line crossing it exactly at right angles, the right superior angle will appear to your right eye too great, to your left eye too small; the other angles show corresponding deviations. To have an apparently right angle, you must make the vertical line incline by an angle of about $1\frac{1}{4}$ degree for it to appear really vertical; and we must distinguish, therefore, the *really vertical* lines and the *apparently* vertical lines in our field of view.

There are several other illusions of the same kind, which I omit because they alter the images of both eyes in the same manner and have no influence upon binocular vision; for example, vertical lines appear always of greater length than horizontal lines having really the same length.

Now combine the two sides of the figure into a stereoscopic combination, either by squinting, or with the help of a stereoscope, and you will see that the white lines of the one coincide exactly with the black lines of the other, as soon as the centres of both the figures coincide, although the vertical lines of the two figures are not parallel to each other.

Therefore not the *really vertical* meridians of both fields of view correspond, as has been supposed hitherto, but the *apparently* vertical meridians. On the contrary, the horizontal meridians really cor-

respond, at least for normal eyes which are not fatigued. After having kept the eyes a long time looking down at a near object, as in reading or writing, I found sometimes that the horizontal lines of the figure crossed each other; but they became parallel again when I had looked for some time at distant objects.

In order to define the position of the corresponding points in both fields of vision, let us suppose the observer looking to the centres of the two sides of the figure, and uniting both pictures stereoscopically. Then planes may be laid through the horizontal and vertical lines of each picture and the centre of the corresponding eye. The planes laid through the different horizontal lines will include angles between them, which we may call *angles of altitude;* and we may consider as their zero the plane going through the fixed point and the horizontal meridian. The planes going through the vertical lines include other angles, which may be called *angles of longitude,* their zero coinciding also with the fixed point and with the *apparently vertical* meridian. Then the stereoscopic combination of those diagrams shows that *those points correspond which have the same angles of altitude and the same angles of longitude;* and we can use this result of the experiment as a definition of corresponding points.

We are accustomed to call *Horopter* the aggregate of all those points of the space which are projected on corresponding points of the retinæ. After having settled how to define the position of corresponding points, the question, what is the form and situation of the Horopter, is only a geometrical question. With reference to the results I had obtained in regard to the positions of the eye belonging to different directions of the visual lines, I have calculated the form of the Horopter, and found that generally the Horopter is a line of double curvature produced by the intersection of two hyperboloids, and that in some exceptional cases this line of double curvature can be changed into a combination of two plane curves.

That is to say, when the point of convergence is situated in the middle plane of the head, the Horopter is composed of a straight line drawn through the point of convergence, and of a conic section going through the centre of both eyes and intersecting the straight line.

When the point of convergence is situated in the plane which contains the primary directions of both the visual lines, the Horopter is a circle going through that point and through the centres of both eyes and a straight line intersecting the circle.

When the point of convergence is situated as well in the middle plane of the head as in the plane of the primary directions of the visual lines, the Horopter is composed of the circle I have just described, and a straight line going through that point.

There is only one case in which the Horopter is really plane, as it was supposed to be in every instance by Aguilonius, the inventor of that name—namely, when the point of convergence is situated in the

middle plane of the head and at an infinite distance. Then the Horopter is a plane parallel to the visual lines, and situated beneath them, at a certain distance which depends upon the angle between the *really* and *apparently* vertical meridians, and which is nearly as great as the distance of the feet of the observer from his eyes when he is standing. Therefore, when we look straight forward to a point of the horizon, the Horopter is a horizontal plane going through our feet—it is the ground upon which we are standing.

Formerly physiologists believed that the Horopter was an infinitely distant plane when we looked to an infinitely distant point. The difference of our present conclusion is consequent upon the difference between the position of the *really* and *apparently* vertical meridians, which they did not know.

When we look, not to an infinitely distant horizon, but to any point of the ground upon which we stand which is equally distant from both our eyes, the Horopter is not a plane; but the straight line which is one of its parts coincides completely with the horizontal plane upon which we are standing.

The form and situation of the Horopter is of great practical importance for the accuracy of our visual perceptions, as I have found.

Take a straight wire—a knitting-needle for instance—and bend it a little in its middle, so that its two halves form an angle of about four degrees. Hold this wire with outstretched arm in a nearly perpendicular position before you, so that both its halves are situated in the middle plane of your head, and the wire appears to both your eyes nearly as a straight line. In this position of the wire you can distinguish whether the angle of the wire is turned towards your face or away from it, by binocular vision only, as in stereoscopic diagrams; and you will find that there is one direction of the wire in which it coincides with the straight line of the Horopter, where the inflexion of the wire is more evident than in other positions. You can test if the wire really coincides with the Horopter, when you look at a point a little more or a little less distant than the wire. Then the wire appears in double images, which are parallel when it is situated in the Horopter line, and are not when the point is not so situated.

Stick three long straight pins into two little wooden boards which can slide one along the other; two pins may be fastened in one of the boards, the third pin in the second. Bring the boards into such a position that the pins are all perpendicular and parallel to each other, and situated nearly in the same plane. Hold them before your eyes and look at them, and strive to recognize if they are really in the same plane, or if their series is bent towards you or from you. You will find that you distinguish this by binocular vision with the greatest degree of certainty and accuracy (and indeed with an astonishing degree of accuracy) when the line of the three pins coincides with the direction of the circle which is a part of the Horopter.

From these observations it follows that the forms and the distances of those objects which are situated in, or very nearly in, the Horopter, are perceived with a greater degree of accuracy than the same forms and distances would be when not situated in the Horopter. If we apply this result to those cases in which the ground whereon we stand is the plane of the Horopter, it follows that, looking straight forward to the horizon, we can distinguish the inequalities and distances of the different parts of the ground better than other objects of the same kind and distance.

This is actually true. We can observe it very conspicuously when we look to a plain and open country with very distant hills, at first in the natural position, and afterwards with the head inclined or inverted, looking under the arm or between our legs, as painters sometimes do in order to distinguish the colors of the landscape better. Comparing the aspect of the different parts of the ground, you will find that we perceive very well that they are level and stretched out into a great distance in the natural position of your head, but that they seem to ascend to the horizon and to be much shorter and narrower when we look at them with the head inverted: we get the same appearance also when our head remains in its natural position, and we look to the distant objects through two rectangular prisms, the hypothenuses of which are fastened on a horizontal piece of wood, and which show inverted images of the objects. But when we invert our head, and invert at the same time also the landscape by the prisms, we have again the natural view and the accurate perception of distances as in the natural position of our head, because then the apparent situation of the ground is again the plane of the Horopter of our eyes.

The alteration of color in the distant parts of a landscape when viewed with inverted head, or in an inverted optical image, can be explained, I think, by the defective perception of distance. The alterations of the color of really distant objects produced by the opacity of the air, are well known to us, and appear as a natural sign of distance; but if the same alterations are found on objects apparently less distant, the alteration of color appears unusual, and is more easily conceived.

It is evident that this very accurate perception of the form and the distances of the ground, even when viewed indirectly, is a great advantage, because by means of this arrangement of our eyes we are able to look at distant objects, without turning our eyes to the ground, when we walk.

All the lunatic asylums in Canada are represented as overcrowded. A new private one has lately been established, on the St. Foy Road, near Quebec, by Mr. Wakeman, formerly and for many years of the Beauport Asylum.

Bibliographical Notices.

The Nervous and Vascular Connection between the Mother and Fœtus in Utero. By JOHN O'REILLY, M.D., F.R.C.S.I. Revised and Enlarged. *Γνῶθι σεαυτον.* New York: printed by Robert Craighead. 8vo. Pp. 86. 1864.

WE scarcely know how to begin a critical review of this pamphlet, the back cover of which is filled with so many flattering notices and recommendations of it. If we were to begin where criticism first suggests itself, we would allude to the expression in the title-page of "the Mother and Fœtus in Utero," and remark that we never knew of their being there at the same time. We should next ask the appositeness of the motto. To which does it apply—to the mother or to the fœtus? We happen just now to be neither. When we were the one we could not have read the pamphlet had it been written, and owing to our sex we cannot ever expect to be the other. The opening sentence in the introduction is very gratifying. "The ennobling profession of surgery is rapidly rising to the highest pinnacle of perfection and usefulness."

The next that follows is worthy, too, to know. "Medicine is brilliantly advancing in the sciences of diagnosis, prognosis and pathology, but is yet in its infancy in point of knowledge with respect to the etiology of diseases," &c. &c. And then the object of the book is stated. "In the following pages I have attempted to throw light on many recondite, abstruse and hitherto unexplored matters."

The work itself commences with the question, "Is there a nervous connection between the mother and fœtus *in utero?*" (meaning, of course, "between the mother, and *the* fœtus in utero.") To answer this question the author has collected a great number of facts, and discusses the assertions of others, but we have to confess we do not always see the application of his facts or the appositeness of his discussions, either as to matter or manner. He asserts that the connection is by means of nerves of the ganglionic system passing along or in the coats of the arteries. In arguing for the truth of this assertion, he brings forward facts which *he thinks* can only be explained by such a theory—to us a very unsatisfactory way of handling the question. His analogies, too, are by no means unimpeachable. For instance, he reasons that as an impression upon the toe is transmitted to the brain by nerves, so an impression upon the mother must be transmitted to the fœtus by nerves; and in expatiating upon this point we have some ten pages upon the nervous system in general—its anatomy, histology and physiology, which might just as well be anywhere else. They do not exhaust the subject either in statement or argument, and for all useful purposes just here the deductions of the author might have been given, leaving the reader to accept them or not, according to his own knowledge and reason. One of the facts cited in proof of the nervous connection between the mother and fœtus is in the quotation from St. Luke, "As soon as the voice of thy salutation *sounded* in *my* ears, the infant in my womb leaped for joy"; but fearing that there are some persons who do not believe in the Gospel, several cases are cited from the author's own practice and from that of others to show that impressions upon the mother are felt by

the fœtus—a fact that we cannot conceive any physician should deny. We cannot continue to examine this book in detail. We find in it Genesis and geology quoted to show that "The regulation of the laws of life, the preservation and propagation of life, devolve on the organic nervous system." Then we have two more pages of note, in small print, that take a general review of a good many things and state a good many facts which we had long known, though we should not have expected to have found them here. On the next page a very common but important error is corrected. "Instead of saying there are no organic nerves in the arteries of the brain, he must rather say the arteries are altogether composed of the organic nervous matter, what is called and constitutes *nervi molles*." (P. 24.) Then we are told "No man living could point out in a section of the placenta the various vessels which enter into its structure." Evidently some of our friends have tried this too soon. They ought to have waited until they were "down among the dead men." A little further on we have a fair specimen of the author's method of induction. "The liver purifies the blood, the placenta purifies the blood—the functions of the liver and [of the] placenta are, therefore, analogous. The number of vessels in each organ being the same, and the anatomical organization of the liver being settled, it follows, as a consequence, that the same anatomical description must be applicable to the placenta." *Ex pede Herculem.*

Part Second has a good deal of chemistry in it, besides other things. The Appendix contains a large number of cases illustrating the influence of the mother while pregnant upon her offspring—one showing how a negro and mulattress can get a white child, provided the woman is sufficiently impressed with the *idea* of a white man. There are also several interesting wood-cuts—a man born with the head of an elephant, a horse with a girl's head, &c.

The author has certainly gone over a large field, and wandered very incontinently into several other large fields. He has picked up many interesting facts and indulged in many fanciful imaginations—has read much and quotes much, but we cannot feel he has logically answered the question he commenced with, or sensibly enlarged the knowledge his motto specifies.

W. E. C.

THE BOSTON MEDICAL AND SURGICAL JOURNAL.

BOSTON: THURSDAY, NOVEMBER 10, 1864.

The Introductory at the Massachusetts Medical College.—We have all, old and young, once been students, and can recall the pleasure with which, in those days, we looked forward to the opening lecture of the winter course. Fresh from the quietness of country homes or the private instruction which the city afforded, and filled with an ardor for learning and the firmest resolutions of industry, against which, alas! the stern reality of four months of hard, pine benches proved too strong, we came together upon this occasion, eager to re-

new old acquaintanceship and curious to study the new faces of those who were to be our fellow-students during the semester. To many it was the first opportunity of visiting the halls where degrees were won or of seeing a professor, and to all of us, we think, the words then spoken conveyed a more lasting impression than any which followed. For ourselves we still like to attend these yearly openings, to see the gathering of the young men who are to succeed us, and to be reminded, by the words we hear, of how much there is still to be learned in our art before we cease to be students.

The introductory lecture of the present season, which was given at the College last week by Prof. Clarke, was specially adapted to make us feel not only the necessity, but almost the hopelessness, of making ourselves acquainted by study with the advance of science since our student days. It was a clear and forcible exposition of the progress which has been made in our knowledge of materia medica during the present century. The improvements which have been introduced into pharmacy by the aid of chemistry, the minute analysis of the physiological action of drugs, and the enlarged scope which is now given to therapeutical agents, were all considered and strongly contrasted with the customs and ignorance of comparatively recent days.

The opinion may be entertained outside our immediate vicinity and among those who have no better means of judging than what they have heard or read, that a general scepticism in therapeutics prevails among the profession in Boston. It at least would not be surprising if such a belief did exist, but we can assure our readers there is no real foundation for such an opinion. Something, indeed, has been written and said by individual physicians, but the number of these may be counted upon the fingers of half a hand, and their words when analyzed, like those of a Van Winkle, are adapted only to an audience and a science of half a century ago. To such sweeping notions there can be no better answer than the daily practice of every intelligent and educated physician among us: for eyes and understanding are given to one as well as to another. There can certainly be no doubt as to the entire orthodoxy of the doctrines taught in the School by the Professor of Materia Medica, or of the popularity of his instruction with the students; and his excellent address, which we shall publish in our next number, may be accepted by our readers as an able exposition of the unchanging importance of rational therapeutics in the treatment of disease, and a fair representation of the views entertained by the profession of New England.

On the Inhalation of Oxygen Gas. — Dr. Richardson, M.A., M.D., read a communication on the "Inhalation of Oxygen." The paper was supplementary to one which he had laid before the section at the meeting of the Association held at Oxford. The author said his experiments on the inhalation of oxygen had led him to an almost precise knowledge of the condition under which oxygen would most freely combine with blood. It had been stated in almost every modern work on physiology, that oxygen inhaled in the pure form, is a narcotic poison. These statements are based on the researches of Mr. Braighton, in which the late Sir Benjamin Brodie took part. The observations of Mr. Braighton, in so far as the recital of the phenomena observed by him were con-

cerned, were strictly correct, but the inferences that had been drawn from him were nearly altogether incorrect, and were, at the best, so narrow, as to be comparatively valueless. In fact, Mr. Braighton had seen but one form of oxygen inhalation. The author next stated that the influence of oxygen in inhalation was modified—1. By dilution of the oxygen. 2. By dilution of the blood. 3. By the activity of the oxygen. 4. By the presence or absence in the blood of bodies which stop combination. On the point of dilution of oxygen, Dr. Richardson stated that a certain measure of dilution was required, not because the body consumed too quickly in pure oxygen, but because neutral oxygen would not combine with the carbon of the blood unless it were diluted. In atmospheric air, the dilution is just sufficient to do no more than alter combination, and the quantity of oxygen may be increased, with absorption at sixty degrees to sixty-five degrees, Fahr., if the oxygen is raised in amount to three parts of the gas, to two of nitrogen. Beyond this, the combining power is reduced, and oxygen is not absorbed. Hence animals die in the gas as it approaches the pure state; they die not by a narcotic process, but by a process of negation. On the point of dilution of the blood, the author said that blood possessing a specific gravity of 1053 seemed to have most steady power in absorbing oxygen, as it existed in common air; by increasing the quantity of water in the blood to a limited extent, say, until it lowered the blood to 1060, the absorption of oxygen is increased to a maximum, and after that it is diminished. Below 1055 degrees the absorption of oxygen steadily declines. In respect to the activity of the oxygen, the most different results are obtained, according to the activity. If the oxygen be made fresh from chlorate of potassa, it sustains life even in the pure form, and the activity of the functions are increased: if electric sparks are passed through the gas, or if the gas be heated to 100°, the same is the fact. On the other hand, if the gas is exposed to ammonia, to decomposing animal matter, or even to living animals, over and over again, it loses, even when diluted, its activity, and no longer combines with the blood. In reference to the last point, Dr. Richardson said that there were conditions of blood in which the power of absorption was limited. Alcohol, chloroform, opium, and certain alkaline products, formed in the blood in disease, prevented absorption of oxygen, and death not uncommonly took place from this cause. Great increase of water did the same. After this description, Dr. Richardson added that the question had often been put, whether the inhalation of oxygen could be usefully applied in the treatment of disease? Priestly, Beddoes, Hill, and many of those who lived when oxygen was first discovered, had formed the most sanguine expectations on this point; they saw before them an elixir, if not *the* elixir vitæ. Chaptal, in speaking of the effects of oxygen in consumption, said of it: it raises hope, but, alas! it merely spreads flowers to the tomb. Since then, various opinions of the extremest kind have been expressed; the differences have arisen from the entire want of order that has been followed in the inquiry. The one man has used pure oxygen, the other diluted. The one has given the gas to anæmic people, whose blood is surcharged with water; the other to diabetic or choleraic persons whose blood is of high specific gravity: the one has given it heated, the other at the temperature of the day. If even a stick of phospho-

rus was exposed to oxygen under such varying conditions, the phenomena obtained would be as variable as those that had been registered in physic regarding oxygen as a remedy. The difficulties of arriving at uniform results, had been almost insurmountable from another cause, that of obtaining oxygen in a practical form for inhalation. Fortunately, this difficulty has now been removed. The discovery by Mr. Robbins of a mode of evolving oxygen, by acting on peroxide of barium and bichromate of potassa with dilute sulphuric acid, had given him (Dr. Richardson) the opportunity of inventing a little apparatus for inhaling oxygen, which could be carried anywhere, and used at a moment's notice. The author here exhibited and described the apparatus. It consists of two glass globes, with a double-valved mouthpiece, connected with the escape tube of one globe. The powder containing the oxygen was placed in one globe, and dilute sulphuric acid was poured on it. The oxygen gas was evolved and passed over into the second globe, which was half filled with water. From this, after being washed, in passing through the water, the gas was inhaled. The apparatus was so arranged that any dilution of oxygen recommended—three parts of oxygen to two of nitrogen—could be secured; and, by changing the water in the second globe, so as to have hot or temperate or very cold, the activity of the combination could be graduated. In conclusion, Dr. Richardson remarked that his object in bringing forward this short communication was to invite medical men to a method of research which promised much, and which now might be carried on with certainty of result and uniformity of experience.

Professor Wanklyn was prepared, as were many others, for the fact mentioned that it was a matter of little consequence whether much or little oxygen existed in a certain quantity of air—that the effect was deteriorated by its freshness—by the presence, in fact, of very minute quantities of, perhaps, ozone, or oxide of hydrogen. In operating with what seemed to be pure oxygen, they were operating with a gas containing large quantities of very active substances, and there was reason to believe that it was those substances, and not the oxygen, that produced the effects in breathing which were formerly attributed to oxygen. Take, for instance, a large room filled with people, close the doors and windows, and, as they knew, in half an hour's time the air would become much deteriorated. It was to be supposed that this deterioration was due to the abstraction of oxygen and the evolution of carbonic acid. It had now been ascertained, however, that this had nothing to do with the result, and, if they took a sample of air from the most crowded theatre, the relative proportions of the oxygen to carbonic acid would not be found to have altered more than one part in 1000 of the normal condition. To what, then, was the deterioration due? Carbonic acid, so far as they knew, was not so particularly active that one part in 1000 might determine whether the air was a little better or worse. No doubt human beings gave off minute quantities of extremely poisonous substances, which did deteriorate the air to such an extent as to remove every trace of oxygen from it. It was commonly supposed that in the Black Hole at Calcutta the loss of life arose from the want of oxygen; but that was not the case, for at the time the air did not contain over 1 per cent. less oxygen, or more carbonic acid than under ordinary circumstances.

The President observed, that Sir Humphry Davy, in his early experiments, discovered that, by inhaling pure oxygen, there was no more change in the system than with the ordinary oxygen of the atmosphere, and more recent experiments had shown that the proportions of oxygen and carbonic acid were only capable of a certain extent of alteration. The important point appeared to be, not that there should be a certain proportion of oxygen, but that which existed should be fresh. We had made an analysis of the air in a printing office in the metropolis, both by day and by night, and we found that in the daytime there was more than double the amount of carbonic acid gas free than there was in the external air; and this proportion was again doubled at night. Some of these offices were crowded with persons night and day, and the consumption was from 300 to 400 feet per man, allowing for gas burners. There was good evidence to show that there were conditions in which a very large increase of carbonic acid existed in the air after respiration and combustion.

Dr. Richardson:—Of course in rooms such as those there might be a large accumulation of carbonic acid, and persons might be influenced by it, but the fact he wished to lay down was, that carbonic acid was not necessarily the cause of deterioration, and that oxygen itself by constant use might be brought to such a condition as not to sustain life. In one of his experiments an animal, placed in oxygen, after remaining for some time showed signs of a sinking condition, such as poor people living in badly ventilated houses exhibited, but upon charging the air in the vessel with electric sparks the animal recovered. These facts might be turned to account, and a wholesome atmosphere produced by using the preparation to which he had referred, or the bichromate of potassa, placed in open vessels and treated with sulphuric acid, producing ozonized oxygen. They might ventilate a room in the best possible manner, but if the supply of air passed over a bad neighborhood they might be only the worse for their precautionary measures. Then, again, the condition of the blood had a good deal to do with the absorption of oxygen. Pale persons, whose blood was exceedingly watery, did not absorb oxygen to the same extent as others, and thus fainting was produced.—*From Proceedings of British Association.*

Permanganate of Potash in Gonorrhœa. By John G. Rich, M.D., Beachville, Canada West.—We find the following among the selected articles of the *Chicago Medical Journal*:—

For the last two years I have frequently employed the permanganate of potash as an injection in the treatment of gonorrhœa, and the constant success derived from its use has been extremely satisfactory. My usual method had previously been to administer, first, a hydragogue cathartic, then to give a mixture of cubebs, copaiva, nitre, &c., with injections of sulphate of zinc, tannic acid, &c. But since employing the permanganate my treatment has been much more circumscribed, for with this remedy alone I have frequently cured very bad cases in forty-eight hours, and this, too, without its being followed by any evil effect from the sudden arrest of the discharge. My usual mode of treatment, however, is as follows:—℞. Potassæ bitart., ℈i.; podophyllin, gr. i. M. In chartulas quatuor dividendus. S. One every

two hours until free catharsis is produced. After which: ℞. Potassæ permangan., gr. vi.; aquæ fontan., ℥i. M. S. To be used as an injection three times a day. I direct at the same time the free employment of mucilaginous drinks, as althæa, ulmus, acacia, &c., and put the patient upon a non-stimulating regimen.

Out of sixty-four registered cases this course of treatment has failed in but two instances; and I find that recent attacks usually become arrested by it after from three to six injections. I have found it advisable to continue the demulcents for at least a week after the cessation of the discharge. In none of all these cases was the injection continued after the fourth day.

When accompanied by chordee, I usually employ the following:— ℞. Lupulin, ℈iss.; pulv. camphoræ, ℈i.; micæ panis, q. s. M. Ft. mass in pilulas, xvi., dividenda. Two, three, or four on going to bed.

I think that the permanganate of potash is a remedy deserving of more notice than physicians have hitherto given it, and I hope that my experience may produce for it a more extended trial in cases of gonorrhœa.

VITAL STATISTICS OF BOSTON.

FOR THE WEEK ENDING SATURDAY, NOVEMBER 5th, 1864.

DEATHS.

	Males.	*Females.*	*Total.*
Deaths during the week - - - - - - - - -	38	41	79
Ave. mortality of corresponding weeks for ten years, 1853—1863,	35.2	35.6	70.8
Average corrected to increased population - - - - -	00	00	76.12
Death of persons above 90 - - - - - - - - -	1	0	1

BOOKS RECEIVED.—A System of Surgery; Pathological, Diagnostic, Therapeutic and Operative. By Samuel D. Gross, M.D., Professor of Surgery in the Jefferson Medical College of Philadelphia, &c. Illustrated by over thirteen hundred Engravings. Third Edition, much enlarged and carefully revised. In two Volumes. Philadelphia: Blanchard and Lea. 1864.—Diphtheria; its Nature and Treatment, with an account of the History of its prevalence in various Countries. By Daniel D. Slade, M.D. Being a Second and Revised Edition of an Essay to which was awarded the Fiske Fund Prize of 1860. Philadelphia: Blanchard and Lea. 1864.—The Army Surgeon's Manual, for the use of Medical Officers, Cadets, Chaplains, and Hospital Stewards; containing the Regulations of the Medical Department, all General Orders from the War Department, and Circulars from the Surgeon-General's Office, from Jan. 1st, 1861, to July 1st, 1864. By William Grace, of Washington, D. C. New York: Bailliere Brothers. 1864.—Collection de Mémoires sur une Fonction Méconnue du Pancréas la Digestion des Aliments Azotés. Par Lucien Corvisart, Médicin Ordinaire de l'Empereur, Officier de la Légion d'Honneur, Lauréat de la Faculté de Médecine et de l'Institut, &c. Paris: Victor Masson et Fils. 1857-1863.

MARRIED,—At the residence of the bride's father, Oct. 27th, Dr. Daniel Mayer, of Pomeroy, Ohio, to Miss Addie Walker, of Brownstown, Va.—In Concord, Oct. 31, Dr. Robert W. Wood, of Honolulu, Hawaiian Islands, to Miss Lucy Jane Davis, of Concord.

DIED,—In Providence, Oct. 29, Dr. Richard Brownell, in his 75th year. He was noted as a good man and skilful physician.—At New Orleans, 21st ult., at the U. S. Naval Hospital, of yellow fever, Ira W. Bragg, Asst. Surgeon U. S. Navy, formerly of Croydon, N. H., 28.

DEATHS IN BOSTON for the week ending Saturday noon, Nov. 5th, 79. Males, 38—Females, 41.—Accident, 3—aneurism, 1—apoplexy, 1—inflammation of the brain, 1—congestion of the brain, 1—disease of the brain, 2—bronchitis, 4—cancer, 2—cholera infantum, 3—consumption, 13—croup, 3—debility, 2—diarrhœa, 1—diphtheria, 3—dropsy, 1—dropsy of the brain, 2—dysentery, 3—erysipelas, 1—scarlet fever, 5—typhoid fever, 1—disease of the heart, 3—inflammation, 1—congestion of the lungs, 1—inflammation of the lungs, 2—marasmus, 2—old age, 3—paralysis, 2—peritonitis, 1—puerperal disease, 2—disease of the skin, 1—smallpox, 1—sore throat, 1—teething, 1—thrush, 1—unknown, 5.

Under 5 years of age, 27—between 5 and 20 years, 10—between 20 and 40 years, 14—between 40 and 60 years, 9—above 60 years, 19. Born in the United States, 53—Ireland, 18—other places, 8.